D-1

NICKLES

2ND DRAFT COMMENTS

Comments "Comparison Studies – Draft No. 2
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1. Paragraph numbers sure would make commenting easier. Since there are no numbers I will describe the location by page number and paragraph on that page. Noted -- no change.

2. *Page 1-6, 1st Par, last sentence*; It may be true that discharge data is not available for all of the models, but you should state the whole truth. If you had discharge data for every minute you still could not use the data since Micro-Model (MM) technology ignores it. The prototype data and WES model data in this report has associated stage and discharge hydrographs and I expect that most of the MM study reaches have both water surface and discharge data that was not obtained because it is not used. I know for fact that for the studies of the Kate Aubrey reach, there is plenty of discharge data available. Since the MM technology cannot scale nor reproduce stages nor discharges, it is wrong to blame "lack of data" as the excuse for ignoring the single most contributor (stage/discharge) to a river bed form. It is the shortcoming of the MM technology not lack of data….and should be reported that way. Bankfull discharges and discharges at +20, and 0 LWRP were not available from data provided on WES models or the prototypes. The lack of discharge data, whatever the reason, meant that the geomorphic variable of Q could not be analyzed in any meaningful way for this study. This report simply presents a comparison of data between large-scale (WES) and small-scale (micromodels) and their respective prototypes. There is no direct comparison of any model to another presented in this report. The prototype serves as the sole basis of the comparisons. Any inferences drawn by comparing micromodel and WES model from this study are the sole possession of the reader. Similarly, this report does not attempt to document the WES or micromodel methodology or technique. The ability (or inability) of the micromodel technique to relate model discharge to prototype discharge is also not at issue because data were not available to consistently analyze how the micromodel discharges relate to the prototype discharges. Limitations of the micromodel that may exists are to be documented in the subsequent companion report. I believe CN has a point. I suggest adding a sentence before the last sentence such as "The mm has not used discharge in design and operation of the model and was not measured in most previous mm studies." added sentence

3. *Page 1-6, 3rd Par, all*; The Webster's dictionary defines thalweg as

   "**1a:** a line following the lowest part of a valley whether under water or not **b:** the line of continuous maximum descent from any point on a land surface or one crossing all contour lines at right angles **c:** subsurface water percolating beneath and in the same direction as a surface stream course **2:** the middle of the chief navigable channel of a waterway which constitutes a boundary line between states"

This definition allows a lot of latitude to the modeler to define the location of the thalweg. From page 1-4 you state "the real comparisons of whether a model was considered calibrated/verified depended on the visual interpretation of the model or prototype bathymetry by the respective modeler(s) as opposed to any rigorous technique. The current research identified a need for quantitative comparison data to facilitate evaluation of the morphologic similarity criteria." It seems to me that you are substituting one interpretive method with an other. I more closely followed part **2:** of the above definition whereas someone else could connect deep point to deep point for each cross-section. Both could be considered right. I think you should make it clear that the thalweg location is **qualitative** not quantitative. Comment noted and attempt will be made to clarify.

4. *Page 1-8, 2nd Par, last line*; I do not agree with this statement. For a historic calibration like used for the WES models, a large degree of change between the beginning and ending surveys is good. This indicates the reach is active and is dominated by the hydrograph (stage/discharge), which gives the modeler a clue to the parameters that need to be adjusted. Also surveys taken during high water or low water could differ greatly. I can see where a high degree of variability could make MM calibration difficult. I could not have any confidence in a MM calibrated to highly variable surveys, because the regime of the model and prototype are so different. A high degree of variability indicates to me a unstable reach which means the sediment load entering and leaving the reach are not stable whereas the MM results are (supposedly). This statement links prototype variability to the difficulty in modeling a particular reach. Likewise, calibration of ANY model is complicated by higher prototype variability. Does your experience tell you that it is easier to achieve verification when the "parameters" require more adjustment than when they don't?

This is an issue I have tried to bring up before but have not done a good job of explaining it. To the mm, variability is a problem. To a model that attempts to replicate hydrographs, variability may be a problem or it may be explained by the intervening hydrograph. I suggest we make this distinction. ? changes as shown

5. *Page 2-1, 2.1 Methodology, all*; How did you account for the slope of the LWRP when using the MM data. For example in the WES Kate Aubrey model there was 8 total feet of slope in the LWRP over the reached modeled and it was not evenly distributed. The prototype surveys and the WES models adjusted the elevation referenced to LWRP as the LWRP changed. How was this done for MM data? LWRP was not adjusted in micromodel studies. A single point conversion to LWRP was used. This conversion utilized a linear LWRP as opposed to a non-linear LWRP that exists in the prototype. Based on my knowledge the MM is surveyed as a distance below the top of the insert and the elevation is of "0" is set by the shift. Which means that 0 LWRP is a fixed distance below the top of the insert and the only slope in it is the table slope. You can in no way assume the table slope is equal to the LWRP slope, because the prototype LWRP slope is

relative to the channel centerline, whereas the slope in the MM channel is relative to the direction the channel is running on the table. For example in the New Madrid MM, the table had positive slope but because of the channel orientation on the table most of the channel along the centerline had no slope and in places a negative slope. Also supplementary slope is sometimes required to make the model sediment move and must be removed from the survey for comparison to the prototype. If you consider the Kate Aubrey reach model in the WES model, if the parameters you calculate that are based on the cross-section shape below an el. referred to the LWRP are not adjusted for the LWRP slope (to match the prototype), then the parameter are invalid to compare to the prototype or WES models whose calculated parameters are adjusted for the LWRP slope. If the MM cross-sections were not adjusted to reflect the LWRP slope, then any calculated parameter derived from an area or length of section below a specific LWRP elevation compared to the same parameters from the prototype or WES model cross-section are **TOTALLY INVALID.** And should be deleted from this report. ANALYSIS IS VALID based upon the following. Although micromodels use only one LWRP conversion (through a linear LWRP slope), this does not invalidate the comparison. The comparison of bathymetric data derived from the present/past micromodel procedure to prototype bathymetry remains a valid approach for assessing how well a model reproduced the prototype behavior. Errors introduced into micromodel elevations by not utilizing a variable sloped LWRP (as exists in nature) may influence the level of agreement, but the error would generally be to lessen the differences (and result in a better agreement). If previous micromodel bathymetry were corrected to adjust for varying LWRP slope over the length of the model reach, it is possible that a different shift would have been used in achieving a calibrated base test. Therefore, it is not possible to apply corrections to the available micromodel bathymetric data. Nor is it possible to determine the extent or magnitude of error introduced to each micromodel as a result of using a single point LWRP conversion. A further compounding issue regarding error magnitude is that recent prototype surveys involve a single point adjustment between absolute elevation and LWRP elevation for individual hydrographic survey sheets (one sheet is ~ 3-4 miles of river). This results in a stepped LWRP over the model reach.

It seems to me that if we know that the current approach of using the table top slope as our reference introduces some error, we should state that and move on. Maybe in report where we discuss shift.— this report does NOT even mention shift.

6. *Page2-22,Par3,sent. 1*; All the models in this report except for the MMs did reproduce bankfull stages and higher, give me some examples of models other than MMs that use a hydrograph type input that did not reproduce bankfull stages. Also the you stated that the MM typically reproduce stages up to +20 LWRP, I question this since stage is not controlled nor measured in a MM, show me some data to prove me wrong. If you cannot prove it with valid data then do not state it!! Noted. Here's the data....Micromodel dike elevations are set to approximate LWRP elevations (typically +15) using scaled prototype elevations. These elevations are continually adjusted during the course of calibration until the "final" shift and vertical scale are developed for the model. Model flow at

maximum discharge overtops these dike structures, ensuring at least a +15 stage in the micromodel. Point checks with the digitizer also serves as confirmation of maximum stages in the range of +20 LWRP. CN first comment was about other models. I agree that MM is only one not doing bankful that I am aware of. Change to reflect that. change "many l. bedm." to micro models

7. *Page 2-22 & 23, Last Par*; I am totally disgusted with this whole discussion, I agree the selection of the water surface will change the numbers, **but the fact remains the data does not change.** All you are doing is massaging the data to say what you want. Pick an elevation, use it throughout and stop insulting the reader with this silliness. This is like changing the vertical scale on a graphical plot to make a line straight or wavy, but it does not change the facts!! You used 7 pages of text, one figure and one table to discuss something that is unimportant and does not change the results of this report, all it does is change the magnitude of some numbers. Also if the water surface elevation you select for calculating any of the parameters intersects one or both of the vertical sides of the MM insert, it should not be used. The cross-section is totally dissimilar to the prototype because the bank line was formed by a saw cut that on a line drawn by the modeler on a photo that is independent of elevation and is cannot be considered accurately similar to the prototype either location or elevation. Noted. No change.

8. *Page2-23, Last Par*: I think the table reference should be Table 2-4 instead of 2-5. Table incorrectly labeled. Correct to show Table 2-5 on page 2-26.

9. *Page2-24,Par 1, Sent. 1*; You stated "there is more agreement between model and prototype surveys using an elevation of +20 LWRP than when using elevation 0 LWRP." How in the world do you get **more agreement** if you are using the same two cross-sections, impossible? The agreement **did not change, you** changed a number. I again reiterate my comment no. 7. If you have 2 apples and cut them in half, you still have 2 apples. True you have more pieces but still only 2 apples. Should state smaller differences result. Will clarify.

10. *Page 2-27, Part 2.2.4.*; I read this and Appendix A, and have one question "What?" If it took a paragraph and a 15 page Appendix with numerous data calculations to describe a "simplistic approach", I am glad it was not a complicated approach! Really, it seems to me that if you compare identical section of the model and prototype cross-sections you would be okay and could eliminate Appendix A. Simplistic approach is depicted by Figure A-1. The remainder only illustrates the effects that result from varying degrees of truncation.

11. *Page3-2, Par 2, Sent. 3*; I thought the ranges were the same for prototype and all models. I know the ranges in the WES model coincide with the prototype survey ranges of 1975 and 1976. The WES model data (verification and 1975 and 1976 prototype) were analyzed with different ranges than used for analysis of the micromodels. The WES model analysis shows only 28 ranges (Table 3-6). With

prototype ranges at intervals of roughly 1000 feet, this represents a total length of only 5.3 miles. The 70+ ranges used in the micromodels represent a total length of 13 miles. Although I have known previously that different reaches were used in the two models, I did not realize the magnitude of 13 versus 5.3 miles. This and the use of the 1973 survey makes any comparison of these models invalid. The statement on page 4-1 where we put differences between models in the same sentence or on the same page needs to be removed. These differences need to be highlighted to the reader. clarification added. Strike sentence on pg 4-1 "For example ..." to end of ¶

12. *Page 3-2, Sect. 3.1.2*; See Sect 3.1.2-redo.doc for suggested changes. Noted. Changes adopted with modification as shown (See para 3.1.2 folloing these comments).

13. *Page 3-11, Table 3-1, Thalweg Position*; Here again I say a calculated thalweg position is a poor parameter for comparison since its location is subjective to the drawer. I used my own criteria to draw the thalweg position for the Prototype and WES models, but the location was likely not drawn on the MMs using the same criteria. For example, if you look at the WES model verification results, I could have just as easily drawn the crossing further upstream between ranges 20 and 25, thus the model thalweg would have plotted on top of the prototype plots. Also because the WES model bank lines have to be laid back (top of the bank moved away from the channel and the toe moved toward channel) in order to be stable and the MM bank lines are vertical some where near the location of the top of bank, a point to measure from on the either model data that would exactly coincide with a point in the prototype is impossible. Visual assessment and evaluation is the only fair way to compare model and prototype location. Opinion Noted. Emphasizes that care should be used in defining thalweg location. I think if we somewhere note that of the 5 parameters, thalweg is subject to interpretation of analyzer, that will suffice.

14. *Page 3-17, Par 1, 1st Sentence*; You state "Equilibrium in the small-scale models represented the condition where sediment transport and the bed bathymetry remained consistent for successive cycles". MM technology has no method to know if sediment transport is in equilibrium. During my work on the Kate Aubrey model at your lab, Wayne and I did test where we run the model numerous cycles and surveyed after every 3 to 5 cycles. The model did not ever reach equilibrium as described above. It did seem to cycle bed forms every 10 to 15 cycles. This data were recorded and should be in your system unless it has been destroyed. These results show the sediment transport through the model never reaches equilibrium; this makes the modelers decision as to "stability" very critical and should be very careful to be consistent. See remainder of sentence quoted ... "Equilibrium in the small-scale models represented the condition where sediment transport and the bed bathymetry remained consistent for successive cycles (there was no net aggradation or degradation over time observed in the model)." The data mentioned from Fall 1999 were not of a calibrated model because of problems experienced in the model insert. Therefore, any conclusions

or opinions derived from that data may be misleading because the model bed elevations and slope may have been changing during the course of the repetitions.

15. *Page 3-25, Par 1, Sent. 1*; The 1973 survey should never have been considered The 1973 bed form was produced by a hydrograph that cannot in no way be replicated in the MM for either stage or discharge. The 1973 Prototype is in no way a typical (average) bed you can expect in the reach. The one thing this comparison does indicate to me that I would not want to use the model as calibrated to test any alternatives. This data (along with cross-section plots I made of the 1:16,000 scale model and the three prototype surveys) indicate the MM does a fairly good job of reproducing the 1973 bed form, but not the 1975 nor 1976 bed forms. (What plots -- were these of the final calibrated micromodel from Feb. 2001?) I would not want to use a model to evaluate alternatives that most closely reproduces the bed forms caused by the third largest flood of record on the Lower Mississippi River. If you really think about it, the large distortion, steep slope and extremely high velocities that are inherent to the MM would most closely reproduced high flood conditions. Comment Noted. Current data do not support the conclusion that micromodel reproduced 1973 channel. Concur that 1973 should have never been used but I have stated this before. '73 was catalyst of problem, it can not be ignored.

16. *Page 3-36, last Par, 1st Sent*; I think you left out a critical boundary condition, initial bed configuration. Earlier in this report you discuss model and sediment transport be in equilibrium, in this paragraph you tell me sediment transport by nature cannot be in equilibrium. Am I to deduce that the model cannot be in equilibrium either? I cannot read you mind, tell me what you believe and stick to it. Changing sides of the fence in wishy-washy. 1st paragraph page 3-17 states " Equilibrium in the small-scale models … there was no net aggradation or degradation over time." This does not imply that there were no variations in local sediment movement.

17. *Page 3-39, Sect. 3.2.1*; This paragraph needs some work. The way it reads is confusing and difficult to get your meaning. How many cycles were reproduced to develop 022301d (N/A this was the base test)? What is the starting bed form used to initiate series of cycle representations? The thing that jumps out at me when I look at the cross-section plots is for the 022801a (last survey) that in the bend type sections the thalweg is generally the deepest and in the crossing type sections the thalweg is generally the shallowest. This indicated to me instability, not variability. Disagree. I do not remember the exact time frame that I did the sensitivity test, but there should be a large number of surveys associated with, because we ran sowething like 160 cycles. The plots of that data show a definite cycle to the bed form and that it repeated. These surveys do not reflect any of the repeatability runs made in Fall 1999 because of problems with model calibration at that time. The surveys shown begin with a calibrated model bed and then continued simulations by the number of cycles shown in Table 3-8 on pg. 3-39. In all 34 cycles were completed. I also would like to know how many cycles

don't know # cycles, probably 10-15 ... or more; bed after started with previous trial run (survey 022301c) but also incorporate some other modifications.

preceded 022301d and what bed did it start with? The report needs to state that each run cycle was started with the bed from the previous survey.

18. *Page 3-49, Par 1, Sent 2*; Shame-Shame…In research, altering a data set without concrete proof and then using the altered data to influence your conclusion and results is a **BIG NO-NO.** You cannot use the suspect data if you believe it in error but to alter it, you destoryed the integrity of the data set and any conclusions derived from it. There was no editing of the Kate Aubrey data. The Kate Aubrey data were analyzed in their entirety based on a linear interpolation along the cross-section at 100-ft. intervals to obtain data values at the same location (for all surveys) for the variance calculation. Spikes were removed from the JB data set and the narrative says something about manual smoothing. Dave Gordon to respond further on JB variance analysis. Concur that we need to better explain if we are smoothing data by removing spikes.

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3.1.2. Large-Scale Models. An example of large-scale model is the Kate-Aubrey model of the Mississippi River conducted by WES. A photograph of the large-scale Kate-Aubrey physical sediment model is shown in Figure 3-3. The Kate-Aubrey reach is located north of Memphis, Tennessee between river miles 785 and 797. The purpose of the study was to develop a plan that reduced or eliminated the extent of shoaling between river miles 788 and 792.5. The model had a loose-bed of crushed coal sediment material. The model was constructed to scales of 1:300 horizontal and 1:100 vertical (model to prototype, respectively). The coal had a median diameter of 4 mm and a specific gravity of 1.30. Prototype data used in this study were bathymetric surveys for May 1975 and May 1976. Prototype bathymetry for 1975 and 1976 are shown in Figures 3-4 and 3-5, respectively. The model bed configuration and structures (e.g. dike fields) were initially formed (or molded) to the 1975 prototype bathymetry. A model discharge hydrograph was developed from historical stage and discharge records for the prototype from May 1975 to May 1976. The resulting hydrograph (also referred to as the verification hydrograph) was used to simulate the historical period in the model between the two bathymetric surveys. The model discharge was distorted by a factor called the discharge ratio, which is adjusted during the verification period to insure proper bed sediment movement and model bed response. Model sediment material was manually input and recorded at the upstream end of the model during simulations. The rate of sediment input was adjusted during model verification to develop a model stage vs. model sediment input rating curve. The model slope, rate of sediment input, discharge ratio, and boundary conditions (e.g. bank roughness) were adjusted over the course of several repetitions until the final model bathymetry reasonably reproduced the May 1976 prototype conditions. Each repetition began with the May 1975 prototype bathymetry formed in the model. The model was then subjected to the verification hydrograph to obtain a model bathymetry to compare with the May 1976 prototype survey. The large-scale models employed a verification process to establish the model operating parameters. The verification procedure relied on a visual comparison of model and prototype bathymetry as described in Gaines (2002) and was considered verified when the model bathymetry reasonably reproduced the May 1976 prototype condition. Model bathymetry after verification is shown in Figure 3-6.